THURSDAY, DECEMBER 15, 1881

CHARLES LYELL

Life, Letters, and Journals of Sir Charles Lyell, Bart., Author of the Principles of Geology, &c. Edited by his Sister-in-law, Mrs. Lyell. In two volumes. With Portrait. (London: John Murray, Albemarle Street, 1881.)

I.

"THE Principles of Geology" and "The Origin of Species" are the two books which have unquestionably exercised the most powerful influence upon the direction of scientific thought during the present century. The first of these works not only prepared the way for the second, but, as Darwin himself has told us, may actually be regarded as its progenitor, for it was the study of the "Principles" which induced the young naturalist to make his now famous "Voyage Round the World" and to collect those facts and observations out of which eventually grew the theory of Natural Selection. The wonderful revolution in thought which followed the appearance of the "Origin of Species" is still fresh in our minds, but those who could remember the effects produced by the publication of "The Principles of Geology," were wont to relate that fifty years ago scientific thought and speculation received an impetus no less powerful than that of which we have witnessed the results in our own time.

The story of the life of Sir Charles Lyell is the history of "The Principles of Geology," for all Lyell's other scientific writings are either expansions of portions of that great work, or are in some way or other supplementary to it. In the account of Lyell's earlier years we trace the birth and development of the ideas so clearly embodied in this famous book, while by the records of his later years we are reminded of the untiring energy with which he collected materials to expand and illustrate those ideas in the successive editions of the work.

The volumes before us enable us for the first time to trace this interesting story in all its details, and we cannot speak too highly of the skill and judgment with which the editor has arranged the materials at her command. The book consists essentially of Lyell's own journals and letters, a few short explanatory notes on the chief events of his life being interspersed in small type and inserted between brackets, together with a few foot-notes explaining allusions or giving details about persons mentioned in the letters.

Lyell, though born in Scotland, was by descent and education an Englishman. His earlier years were spent either in the New Forest and the towns in the south of England, where he went to school, or at the home at Kinnordy, in Forfarshire, where the family usually spent the autumn. In the south of England young Lyell, whose attention had been from boyhood directed to entomology, had the opportunity of studying the Tertiary deposits of the Hampshire basin; while in Forfarshire the draining of a small loch on his father's property and the excavation of the "marl" with which it had become filled, appear to have early directed his attention to some of the important questions connected with the mode of deposition of strata and the way in which organic remains become imbedded in them—questions afterwards treated by him with such skill and ingenuity in the "Principles." Lyell's two first papers, published in the *Transactions* of the Geological Society, relate to the strata of the Hampshire Basin and the formation of these marl-deposits in the lakes of Forfarshire.

At the age of seventeen Lyell went to Oxford, and there came under the influence of the brilliant and versatile, but eccentric, Buckland. But though impressed with the eloquence and filled with admiration at the energy of his teacher, there is evidence that at a very early date Lyell's mind underwent a revolt against the bold but shallow theorisings of the Oxford professor. When a few years later Buckland published his "Reliquiæ Diluvianæ," we find the pupil not only in open opposition to the master, but actually leading the attacks of the "Fluvialists" against the great stronghold of the "Diluvialists."

Upon leaving Oxford, Lyell was destined for the bar, but he, after reading law for a short time, was obliged to desist on account of the weakness of his eyesight. Under these circumstances he repaired to Paris, where he had the opportunity of constant intercourse with Cuvier, Brongniart, Humboldt, Constant Prévost, and the other brilliant scientific thinkers who were at that period assembled in the French capital. He at the same time studied with care the Tertiaries of the Paris basin, comparing the strata and their fossils with those with which he was already so familiar in Hampshire.

Lyell had now become so thoroughly engrossed in scientific work that all idea of advancement at the bar was abandoned by him, and after going two years upon the Western Circuit, he seems to have finally relinquished law for literature and science. He first began to write in the *Quarterly Review*, having formed a close friendship with Lockhart, then editor of that journal, and, after some papers upon educational questions, he in 1827 undertook a review of Scrope's "Geology and Extinct Volcanoes of Central France." It was in this work that Lyell first showed how entirely he had adopted the principles enunciated by Hutton and Playfair, and how far he was in advance of his most eminent contemporaries, Buckland and Sedgwick in England, and Cuvier and Humboldt on the Continent.

The five years from 1825 to 1830, during which Lyell was maturing his literary style by writing for the reviews and collecting the materials for his great work, may be regarded as the turning-point of his career, and the letters written by him at this period are of the greatest interest to the historians of science. We cannot forbear from making a few extracts illustrating the nature of his work and his views at this period. On June 22, 1826, he wrote to his friend Dr. Mantell—

"I must not sport radical, as I am become a Quarterly Reviewer. You will see my article just out on 'Scientific Institutions,' by which some of my friends here think I have carried the strong works of the enemy by storm. I am now far on with a second, and hope to get it out in less than three months. I mean to help myself out of Cuvier largely, for I must write what *will be read*" (vol. p. 164).

On March 2, 1827, he writes to the same correspondent as follows:—

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"I devoured Lamarck *en voyage*, as you did Sismondi, and with equal pleasure. His theories delighted me more than any novel I ever read, and much in the same way, for they address themselves to the imagination, at least of geologists who know the mighty inferences which would be deducible were they established by observations. But though I admire even his flights, and feel none of the *odium theologicum* which some modern writers in this country have visited him with, I confess I read him rather as I hear an advocate on the wrong side, to know what can be made of the case in good hands. I am glad that he has been courageous enough and logical enough to admit that his argument, if pushed as far as it must go, if worth anything, would prove that man may have come from the Ourang-Outang. But after all, what changes species may really undergo! How impossible will it be to distinguish and lay down a line, beyond which some of the so-called extinct species have never passed into recent ones. That the earth is quite as old as he supposes, has long been my creed, and I will try before six months are over to convert the readers of the *Quarterly* to that heterodox opinion" (vol. i. p. 168).

His aspirations concerning his future at that time will be understood from the following extract from a letter written to his father in the same year:—

"I find my wants diminish monthly in proportion as I am more agreeably employed, and if with the willingness to work and industry which I now have, I had any chance of earning what I require by my own exertions, I should be without a care as far as I am myself concerned. But to be willing without avail to work hard, and almost for nothing, is now the fate of many hundreds of barristers, and many millions of our labouring classes, and we must congratulate ourselves at not being among the latter. I am quite clear, from all that I have yet seen of the world, that there is most real independence in that class of society who, possessing moderate means, are engaged in literary and scientific hobbies; and that in ascending from them upwards, the feeling of independence decreases pretty nearly in the same ratio as the fortunes increase. My eyes go on tolerably, and I feel my facility of composition increases, and hope to make friends among those that a literary reputation will procure me who may assist me" (vol. i. p. 171).

Under date of February 5, 1828, he wrote to Dr. Mantell explaining his plans for the work which he had been for some time contemplating:—

"I at first intended to write 'Conversations on Geology'; it is what no doubt the booksellers, and therefore the greatest number of readers, are desirous of. My reason for abandoning this form was simply this; that I found I should not do it at all, without taking more pains than such a form would do justice to. Besides, I felt that in a subject where so much is to be reformed and struck out anew, and where one obtains new ideas and theories in the progress of one's task, where you have to controvert, and to invent an argumentation—work is required, and one like the 'Conversations on Chemistry' and others would not do. It should hardly be between the teacher and the scholar perhaps, but a dialogue like Berkeley's Alciphron, between equals. But finally I thought that when I had made up my own mind and opinions in producing another kind of book, I might then construct conversations from it. In the meantime there is a cry among the publishers for an elementary work, and I much wish you would supply it. Anything from you would be useful, for what they have now is positively bad, for such is Jamieson's 'Cuvier'" (vol. i. p. 177).

In attempting to free geological science from the trammels with which it had become involved by the efforts of well-intentioned but mischievous works, like the "Vindiciæ Geologicæ" and the "Reliquiæ Diluvianæ," Lyell undertook no light or easy task. His letters to Scrope, who had been requested by Lockhart to review the "Principles" in the pages of the *Quarterly*, show very clearly how sensible Lyell was of the difficulties by which he was beset through the nervous susceptibilities of orthodoxy. The fact that the works of Hutton and Playfair had long ago been placed in a social "Index Expurgatorius," and that Scrope's clear and admirable exposition of the Huttonian doctrines, published in his "Considerations on Volcanoes" in 1825, had altogether failed to revive interest in the ostracised works, was full of warning to Lyell. We find him writing to Scrope, while the first volume of the "Principles" was going through the press, in the following terms:—

"I was afraid to point the moral, as much as you can do in the *Q. R.*, about Moses. Perhaps I should have been tenderer about the Koran. Don't meddle much with that, if at all.

"If we don't irritate, which I fear that we may (though mere history), we shall carry all with us. If you don't triumph over them, but compliment the liberality and candour of the present age, the bishops and enlightened saints will join us in despising both the ancient and modern physico-theologians. It is just the time to strike, so rejoice that, sinner as you are, the *Q. R.* is open to you.

"If I have said more than some will like, yet I give you my word that full *half* of my history and comments was cut out, and even many facts; because either I, or Stokes, or Broderip felt that it was anticipating twenty or thirty years of the march of honest feeling to declare it undisguisedly. Nor did I dare come down to modern offenders. They themselves will be ashamed of seeing how they will look by-and-by in the pages of history, if they ever get into it, which I doubt. You see that what between Steno, Hooke, Woodward, De Luc, and others, the modern deluge systems are all borrowed. Point out to the general reader that my floods, earthquakes, &c., are all very modern, also waste of cliffs; and that I request that people will multiply, by whatever time they think man has been on the earth, the sum of this modern observed change, and not form an opinion from what history has recorded. Fifty years from this, they will furnish facts for a better volume than mine. . . ."

"I conceived the idea five or six years ago, that if ever the Mosaic geology could be set down without giving offence, it would be in an historical sketch, and you must abstract mine, in order to have as little to say as possible yourself. Let them feel it, and point the moral" (vol. i. p. 271).

On two points, as has often been pointed out, Lyell may be held to have betrayed weakness in his reasoning in the "Principles." The first of these was that he appeared to accept in the most uncompromising manner the stringent Uniformitarian views of Hutton, leaving no place even for variations in the intensity of causes now operating. In taking this line he was doubtless influenced by fear of making any dangerous concessions to his adversaries the "Diluvialists." His real feelings on the subject may be gathered from a letter in which he replies to the remonstrances of Scrope upon the subject—

"All I ask is, that at any given period of the past don't stop inquiry when puzzled by refuge to a 'beginning,' which is all one with 'another state of nature' as it appears to me. But there is no harm in your attacking me, provided you point out that it is the proof I deny, not the probability of a beginning. Mark, too, my argument,

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that we are called upon to say in each case, 'Which is now most probable, my ignorance of all possible effects of existing causes,' or that 'the beginning' is the cause of this puzzling phenomenon?" "It is not the beginning I look for but proofs of a *progressive* state of existence in the globe, the probability of which is *proved* by the analogy of changes in organic life" (vol. i. p. 270). See also upon the same subject his letter to Whewell in 1837 (vol. ii. p. 2).

The other question upon which Lyell's reasonings in his "Principles" betrayed weakness and inconsistency was that of the cause of the appearance from time to time of new species of plants and animals upon the earth. While stoutly maintaining the sufficiency of existing causes to account for the gradual disappearance of species by extinction, he felt himself compelled to invoke a creative power to introduce the new species as they were required. But, before we blame Lyell for this apparent weakness, we ought to remember that the work of Lamarck, the only serious attempt which had been at that time made to account for the origin of species, though brilliant and suggestive, was full of assumptions and fallacies that could not fail to betray themselves to Lyell's logical mind, and to militate against his acceptance of the theory. Lyell, moreover, saw only too clearly that the origin of man could not be treated of on different principles to that of other species of animals, and to have come into conflict with the prejudices of the day upon such a point as this, would have been to sacrifice all chance of a patient hearing for his arguments in favour of the "good cause" of which he felt himself to be the apostle. A very interesting letter written by him to Sir John Herschel in 1836, shows very clearly that Lyell had even at that early date thought deeply on the question of the origin of species by natural causes.

"In regard to the origination of new species, I am very glad to find that you think it probable that it may be carried on through the intervention of intermediate causes. I left this rather to be inferred, not thinking it worth while to offend a certain class of persons by embodying in words what would only be a speculation. But the German critics have attacked me vigorously, saying that by the impugning of the doctrine of spontaneous generation, and substituting nothing in its place, I have left them nothing but the direct and miraculous intervention of the First Cause, as often as a new species is introduced and hence I have overthrown my own doctrine of revolutions, carried on by a regular system of secondary causes. I have not wasted time in any controversies with them or others, except so far as modifying in new editions some opinions or expressions, and fortifying others, and by this means I have spared a great deal of ink-shed, and have upon the whole been very fairly treated by the critics. When I first came to the notion, which I never saw expressed elsewhere, though I have no doubt it had all been thought out before, of a succession of extinction of species, and creation of new ones, going on perpetually now, and through an indefinite period of the past, and to continue for ages to come, all in accommodation to the changes which must continue in the inanimate and habitable earth, the idea struck me as the grandest which I had ever conceived, so far as regards the attributes of the Presiding Mind. For one can in imagination summon before us a small part at least of the circumstances that must be contemplated and foreknown, before it can be decided what powers and qualities a new species must have in order to enable it to endure for a given time, and to play its part in due relation to all other beings destined to coexist with it, before it dies out. It might be necessary, perhaps, to be able to know the number by which each species would be represented in a given region 10,000 years hence, as much as for Babbage to find what would be the place of every wheel in his new calculating machine at each movement.

"It may be seen that unless some slight additional precaution be taken, the species about to be born would at a certain era be reduced to too low a number. There may be a thousand modes of insuring its duration beyond that time; one, for example, may be the rendering it more prolific, but this would perhaps make it press too hard upon other species at other times. Now if it be an insect it may be made in some of its transformations to resemble a dead stick, or a leaf, or a lichen, or a stone, so as to be somewhat less easily found by its enemies; or if this would make it too strong, an occasional variety of the species may have this advantage conferred upon it; or if this would be still too much, one sex of a certain variety. Probably there is scarcely a dash of colour on the wing or body of which the choice would be quite arbitrary, or which might not affect its duration for thousands of years. I have been told that the leaf-like expansion of the abdomen and thighs of a certain Brazilian Mantis turn from green to yellow as autumn advances, together with the leaves of the plants among which it seeks for its prey. Now if species come in in succession, such contrivances must sometimes be made, and such relations predetermined between species, as the Mantis, for example, and plants not then existing, but which it was foreseen would exist together with some particular climate at a given time. But I cannot do justice to this train of speculation in a letter, and will only say that it seems to me to offer a more beautiful subject for reasoning and reflecting on, than the notion of great batches of new species all coming in, and afterwards going out at once" (vol. i. pp. 467, 469).

It is probable that during later years Lyell receded somewhat from the position he was prepared to take up at the time when he wrote the above. The crudeness of speculation and ignorance of scientific facts which characterised the earlier editions of the "Vestiges of Creation" had in all likelihood not a little to do with this revulsion of thought, while the powerful influence of the leaders of biological thought, Edward Forbes and Louis Agassiz, always exercised in support of the idea of the permanency of species, doubtless had no little weight with Lyell, as it had with nearly all his contemporaries. How readily Lyell welcomed and embraced the views of Darwin as soon as they were published we all know, for he could not fail to see that by incorporation of the theory of natural selection into his work he was for the first time able to make it complete and consistent with itself. It is interesting to read in the volume before us the impressions made upon him by the first reading of the "Origin of Species" in 1859.

"My dear Darwin,—I have just finished your volume, and right glad I am that I did my best with Hooker to persuade you to publish it without waiting for a time, which probably could never have arrived, though you lived to the age of a hundred, when you had prepared all your facts on which you ground so many grand generalisations.

"It is a splendid case of close reasoning and long-sustained argument throughout so many pages, the condensation immense, too great perhaps for the uninitiated, but an effective and important preliminary statement, which will admit, even before your detailed proofs appear, of some occasional useful exemplifications, such as your pigeons and cirripedes, of which you make such excellent use.

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"I mean that when, as I fully expect, a new edition is soon called for, you may here and there insert an actual case, to relieve the vast number of abstract propositions. So far as I am concerned, I am so well prepared to take your statements of facts for granted, that I do not think the *pièces justificatives* when published will make much difference, and I have long seen most clearly that if any concession is made, all that you claim in your concluding pages will follow.

"It is this which has made me so long hesitate, always feeling that the case of Man and his Races and of other animals, and that of plants, is one and the same, and that if a *vera causa* be admitted for one instant, of a purely unknown and imaginary one, such as the word 'creation,' all the consequences must follow" (vol. ii. p. 325.

After the first publication of the "Principles" between the years 1830 and 1833, a great part of Lyell's time and thought was given up to revising, enlarging, and re-writing portions of his book during the twelve editions through which it passed. Although many valuable corrections were made in the original work, its scope and arguments being extended, and the whole fortified with a great wealth of new illustrations, it may well be doubted whether this continual re-editing of the book was not attended with some loss in the symmetry of its arrangement and its literary excellence. In a work relating to such a rapidly-advancing science as geology, this result, much as it is to be regretted, could scarcely be avoided; but many disciples of Lyell, while they refer to the last edition as a store-house of facts, will delight to renew their acquaintance with an old favourite by reading once more the easily flowing periods of the first edition.

We have dwelt at such length upon Lyell's relations to his great work, as illustrated in the interesting volumes before us, that we must defer to a second notice some of the other interesting topics which are suggested by their perusal.

JOHN W. JUDD

ORGANIC CHEMISTRY

Adolph Strecker's Short Text-book of Organic Chemistry. By Dr. Johannes Wislicenus, Professor of Chemistry in the University of Würzburg. Translated and edited by W. R. Hodgkinson and A. J. Greenaway. 8vo. (London: Kegan Paul, Trench and Co., 1881.)

THE new edition of Strecker's text-book by Prof. Wislicenus, published in 1874, is well known as giving a concise and comprehensive view of the state of organic chemistry at the time of its publication, and some useful additions, relating to recent discoveries, have been made by the English translators.

The classification of organic compounds in this, as in all recent works on organic chemistry, is based upon the hydrocarbons. All organic compounds of known constitution are divided into the two great groups, Fatty and Aromatic, and in each of these the saturated hydrocarbons—paraffins in the first, benzene and its homologues in the second,—are first described; next their mono-substitution derivatives: alcohols, ethers, amines, phosphines, &c.; then in succession the di-, tri-, tetra-, &c., derivatives. With regard to this matter Prof. Wislicenus says in his preface: "The most systematic arrangement would be found in the number of carbon-atoms in direct union. In each such group of equal carbon contents the paraffin would come first, next those derivatives in which only a single hydrogen-atom had been replaced, these being arranged according to the valency of the substituting element. Then would follow the di-substitution products. . . Next the tri-substituted paraffins. . . . This order of arrangement is very valuable for the study of organic chemistry, more so however for those moderately acquainted with the subject than for beginners. For the latter I think we cannot dispense with the study of homologous series, especially in the early part of a text-book. In this way alone can the clear differentiation of the various categories be made evident, depending, as they do, not so much on the accumulation of carbon-atoms, as on the nature and amount of the other elements in union." It is worth while to compare these remarks with those made by Roscoe and Schorlemmer in their lately-published "Treatise on Organic Chemistry," at p. 129 of which we read:—"Perhaps the most systematic mode of arrangement would be to commence each group (fatty and aromatic) with a discussion of the hydrocarbons, and then to follow on with a description of the series of substances obtained by the replacement of one, two, three, or more of the constituent atoms of hydrogen. Such a mode of classification, however, labours under the disadvantage that compounds which stand as a rule closely together, as, for example, the alcohols $C_nH_{2n+2}O$ and the acids $C_nH_{2n}O_2$, are thus found widely separated, whilst other groups possessing but little analogy are brought into proximity. Hence it is desirable, alike for the sake of perspicuity as for the purpose of showing the genetic relationships between different bodies, to depart in many cases from such a systematic treatment, and arrange the compounds according as they are derived one from the other." It will be seen from these quotations that each author regards the arrangement adopted by the other as the most systematic, but prefers his own as best adapted to the requirements of the student.

The additions made to the work under consideration by the English editors belong chiefly to the aromatic group, but no mention is made of the recent investigations of Nevile and Winther, published last year in the Journal of the Chemical Society, on the Bromotoluenes, which are especially interesting on account of the light which they throw on the influence exerted by the groups or radicles which have replaced certain hydrogen-atoms in a benzene nucleus, on the position taken up by other radicles which take the place of the remaining atoms of hydrogen. In the series of paraffins there is an omission of the normal Heptane, lately discovered by Dr. Thorpe in the turpentine of *Pinus Sabiniana*; and amongst the nitroparaffins no notice is taken of the Nitrolic acids and Pseudonitroles. Under the organic compounds of boron we miss Dr. Frankland's Ammonioboric methide, $(CH_3)_3B{=}NH_3$ and Diboric ethopentethylate, $(C_2H_5O)_3B{=}B(C_2H_5)(OC_2H_5)_3$, in which boron figures as a pentad; and under guanidine there is no account of the Guanamines, $C_{n+2}H_{2n+3}N_5$, a series of bases discovered by Nencki in 1874 and 1876, and formed by the action of heat on the guanidine salts of the fatty acids.

The translation reads well, and, with the exception of a few instances of somewhat too close imitation of German forms, is expressed in good idiomatic English. There

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